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FUNDAMENTALS OF STATISTICAL DECISION THEORY.

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STATISTICAL DECISION MAKING THEORY PROVIDES A FRAME OF REFERENCE WITHIN WHICH THE COUNSELOR AND CLIENT CAN CONSIDER A WIDE RANGE OF PROBLEMS OF DECISION MAKING. IN THE FACE OF UNCERTAINTY, A SET OF ACTIONS IS AVAILABLE. PREFERENCE AMONG THESE ACTIONS DEPENDS ON THE TRUE STATE OF NATURE WHICH IS GENERALLY UNKOWN. TO GAIN THIS INFORMATION, AN EXPERIMENT CAN BE PERFORMED WHICH WILL YIELD A SET OF DATA. A STRATEGY FOR DECISION MAKING IS A COMPLETE PRESCRIPTION WHICH SPECIFIES THE EXPERIMENT TO BE PERFORMED AND THE TERMINAL DECISION TO BE MADE FOR EACH SET OF DATA COLLECTED. WITH THE USE OF A LOSS TABLE, THE EXPECTED LOSS FOR ANY STRATEGY FOR EACH STATE OF NATURE CAN BE FOUND. COMPARISONS GENERALLY MAKE IT POSSIBLE TO ELIMINATE INADMISSIBLE STRATEGIES. TWO CRITERIA CAN BE EMPLOYED TO SELECT FROM THE REMAINING CLASSES OF ADMISSIBLE STRATEGIES. FIRST IS THE MINIMAX PRINCIPLE WHICH IS BASED ON THE CRITERION OF SELECTING THE STRATEGY FOR WHICH THE MAXIMUM EXPECTED LOSS IS THE SMALLEST. SECOND IS THE BAYES CRITERION WHICH IS BASED ON THE ASSUMPTION THAT THERE IS A KNOWN PROBABILITY DISTRIBUTION OVER THE STATES OF NATURE AND, THEREFORE, THE EXPECTED RISK OF WRONG DECISIONS CAN BE MINIMIZED BY COMPUTING THE AVERAGE OF THE ASSUMED PROBABILITY DISTRIBUTION. THIS ADDRESS WAS PRESENTED AT THE CONFERENCE ON THE TRAINING OF COUNSELORS OF ADULTS (CHATHAM, MAY 22-28, 1965). (RM)

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FUNDAMENTALS OF STATISTICAL DECISION THEORY

Rosedith Sitgreaves*

Introduction. - There is a widespread and growing literature on the structure and process of decision making. A bibliography of papers in this area (1), published in 1958, lists some 450 titles of publications appearing principally in the post-war years from 1945 to 1957. A supplement to this bibliography (2), covering the six years, 1958-63, lists more than 600 titles. The topics represented include, among others, general and theoretical material on decision making, decision making in small groups, community decision making, communications and information handling, and mathematics and statistics in decision making.

In the present paper, we shall be concerned with the last-named topic, and, in particular, with the concepts and notions of statistical decision theory. This theory was originally formulated as a unified framework within which all problems of statistical inference could be handled. With the development in recent years of mathematical models for personal decision making, it has become clear that the concepts are also generally useful as a basis for considering the problem of making rational decisions in the face of uncertainty. To place the presentation in perspective, it seems worthwhile briefly to review some of the history which led to its formulation and development.

Historical background - It is well-known that the term "statistics" was originally applied to data collected by the State, and that the earliest statistical techniques were concerned with the processing and description of census data of one kind or another. With the growth of the scientific method, the techniques of descriptive statistics were adapted and extended to the analysis of objective measurements collected in a wide variety of situations. In most of these situations, a given variable was measured only a fraction of the times it might possibly be measured. For example, in studying the relation between the heights of fathers and the heights of sons, 500 pairs of fathers and sons might be measured, but, clearly, this does not exhaust the pairs of fathers and sons that could be measured. Thus, there arose the concept of the sample for which the measurements were actually available, and the universe or population which the sample represented. In the process, the term "statistics" became associated with numbers computed for the sample, such as the sample mean or the sample correlation coefficient.

The statistics computed for a sample have an inherent instability. That is, the same statistic, e.g., the sample mean, generally tends to have

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a different value in different samples of the same size from the same population, although this instability tends to disappear as the sample sizes become large. However, most researches are conducted with relatively small samples so that techniques of statistical inference are necessary to generalize from the properties of the sample to the properties of the universe. In most cases, the universe rather than the sample is the principal concern of the experimenter.

Traditionally, the techniques of statistical inference have been developed for two basic kinds of problems of generalization. The first is concerned with estimating the numerical properties of the measurements in the universe, on the basis of the information in the sample. The second is the use of the sample data as evidence for supporting or discarding hypotheses about the universes or populations. These hypotheses are generally quantified by making specifications about the probability distribution of the measurements in the populations, and are tested statistically by judging whether the observed data are probable or improbable under these specifications. The statistical tests are constructed in such a way that the probability of falsely rejecting a hypothesis being tested is controlled at a preassigned low level, such as five percent or one percent, and this probability is called the level of significance of the test.

Many of the early techniques of statistical inference were proposed on intuitively reasonable grounds, but for some time, little attempt was made to construct or to codify logical rules for their development and use. A major effort was made in this direction, in the late 1920's and early 1930's, with the formulation of the Neyman-Pearson theory of testing hypotheses and estimation (3, 4). In developing criteria for constructing and evaluating statistical tests, Neyman and Pearson emphasized the fact that the end result of any test is a decision on the part of the experimenter either to reject or not to reject the hypothesis being tested. Two types of error are possible in such decision-making, depending upon the decision made about the hypothesis and the true state of affairs. One type of error occurs if the decision to reject is made and the hypothesis being tested is actually true. A second type of error occurs if the decision not to reject is made and the hypothesis being tested is actually false.

In any statistical test, the probability of the first type of error, i. e., the level of significance, is controlled by the experimenter. However, in judging the validity of a given hypothesis, a number of different statistical tests may be possible, all of them matched on the level of significance. The probability of the second type of error thus depends not only on the true state of affairs (that is, on the fact that the hypothesis being tested is false and some alternative hypothesis is true), but also on the particular test procedure being used for evaluation of the hypothesis. The Neyman-Pearson theory proposes general criteria for selecting and constructing test procedures, based on the performance characteristics of competing tests over a wide range of alternative hypotheses.

In the accompanying theory of estimation, the notion of estimation of population values by means of a confidence interval is central. The idea of interval estimation itself was not new. It is intuitively reasonable that if we want to estimate the value of an unknown quantity, on the basis of partial information, we might prefer to hedge a little in giving the estimate, rather than confine ourselves to a single number. For example, suppose we know the temperature at noon today, and want to say (or estimate) what the temperature will be at noon tomorrow. We might be reluctant to state that it will be exactly the same as the temperature at noon today (at least, in our climate), or in fact, that it will be any other single temperature value, since it seems unlikely that we would be correct. However, we might feel reasonably confident of being correct if we said it would be within 5 degrees of today's temperature, even more confident of being correct if we said it would be within 10 degrees of today's temperature, and almost sure of being correct if we said it would be within 20 degrees of today's temperature. Each of these statements provides a range, or interval of values, for the unknown temperature, which is given with varying degrees of confidence. In general, the greater the confidence, the longer the interval, and in fact the range of temperatures in the last-named interval is so great that although the interval is given with almost perfect confidence, it is practically meaningless.

The Neyman-Pearson theory proposes a method for developing confidence intervals from a set of sample data with the use of tests of significance, and provides a means of evaluating the confidence with which an interval is given in terms of the significance level of the test. Suppose that each possible value of an unknown constant, e.g., the temperature value at noon tomorrow, is, in turn, hypothesized as the true value. Using the sample data and a fixed level of significance, the experimenter tests each hypothesis to decide whether or not the hypothesized value is tenable in the light of the sample data. All of the hypothesized values which are judged tenable by the tests constitute a confidence interval for the unknown constant. The confidence coefficient with which the interval is given is (in percent) 100 minus the significance level used in the series of tests.

Just as we are interested in evaluating the properties of different tests of the same hypothesis, we are also interested in comparing different sets of confidence intervals which may be obtained for the same unknown constant. Here the intervals can be matched on the confidence coefficient with which they are given, and the criterion generally used is the length of the interval. For a fixed confidence coefficient, we would prefer the method that yields us the shortest possible confidence intervals.

The Neyman-Pearson theory represents a mile-stone in the formulation of a logical structure for statistical tests of hypotheses and estimation procedures. Because of the lag between theory and practice, it was not generally taught in applied statistics courses until the 1950's. Currently, it

still provides the rationale for most of the statistical techniques that are used in conventional statistical problems. As early as 1939-40, however, Abraham Wald had begun to formulate a more general theory which would encompass both testing hypotheses and estimation procedures, as well as other statistical problems which do not fit easily into either of these categories. Wald was influenced not only by the work of Neyman and Pearson, but also by developments in econometrics, particularly by the work of von Neumann and Morgenstern. In their book, Theory of Games and Economic Behavior (5), the authors presented a theory of rational economic behavior in terms of games with associated strategies, and introduced the notion of numerical utilities for the consequences of various actions. Wald saw the possible application of many of these ideas to the problems of statistics, and formulated a theory of statistical decision making in terms of a game between the statistician and nature.

It is essentially this theory which we shall discuss here. Wald died in an airplane crash in India in 1950, but his work was continued and extended by a number of others. In general, the theory of statistical decision making has not proved as useful as it was initially hoped in yielding solutions for specific problems. It has proved invaluable, however, as a frame of reference within which to consider a wide range of problems of decision making in the face of uncertainty.

Basic elements of a statistical decision problem. - What constitutes the basic elements of a statistical decision problem? If we say that it is decision making in the face of uncertainty, the statement is so all-inclusive that it is not very meaningful. We might examine first, therefore, what constitutes uncertainty in statistical decision problems. We will begin with a simple, somewhat artificial problem, which nevertheless exhibits most of the elements of a statistical decision problem.

Suppose it is the afternoon before payday and you have only three dollars left. An acquaintance offers you the following bet. He has a coin with a head on one side and a tail on the other. You are to choose one of the faces of the coin and toss it. If the face you have chosen comes up, your acquaintance will pay you three dollars. If the opposite face comes up, you will pay him three dollars. You must, therefore, make a decision to take one of the following actions:

(1) Reject his offer;

(2) Accept his offer and bet heads;

(3) Accept his offer and bet tails.

As a consequence of the first action you neither gain or lose any money. As a consequence of the second or third you may win or lose three dollars depending on the outcome of the toss.

The uncertainty in this situation is introduced by the fact that the outcome of the toss of the coin depends on chance. If the coin is a fair coin, i.e., heads and tails are equally likely, the law of chance governing the outcome of the toss is known to you. On the other hand, suppose you have reason to believe that the coin is biased; that is, it has been ingeniously constructed in such a way that the odds for heads and tails are not the same, and the actual odds, or true state of nature, is unknown to you. The uncertainty in the outcome is now compounded by the fact that not only is the outcome of the toss a chance event, but the particular law of chance in operation is unknown.

Suppose in this compounded situation your acquaintance amends his original offer as follows. For a penny a toss, you may toss the coin as many times as you please to gain information on the true state of affairs, and then make a decision about his original offer. The decision problem is now more complicated because you must decide whether or not to toss the coin to gain information, how many times to toss it for this purpose, and what terminal action to take in the light of the data you have collected. If you decide to toss the coin, you are, of course, deciding to perform an experiment. If, for example, you decide in advance to toss the coin 10 times and then stop, you are deciding on a fixed sample size experiment. On the other hand, if your decision to stop after 10 tosses depends on the results observed in these tosses, you are performing a sequential experiment.

Let us consider a number of possible courses of action.

(1) You decide not to experiment at all and to bet heads.

(2) You decide to toss the coin 10 times, ignore the results of the tosses, and bet heads.

(3) You decide to toss the coin once and to bet on whichever face appears on the toss of the coin.

(4) You decide to toss the coin twice. If two heads appear, you decide to bet heads. If two tails appear, you decide to bet tails. If a head and tail appear (in either order), you decide not to bet.

(5) You decide to toss the coin twice. If two heads appear, you decide to be heads. If two tails appear, you decide to be tails. If a head and a tail appear (in either order), you decide to toss the coin a third time, and bet on the face that appears on the third toss.

Any complete specification of the experiment you will perform together with a rule for deciding on a terminal action is called a strategy. No value judgment is implied in this term. A strategy is neither good nor bad in itself; it is simply a completely prescribed course of action leading to a terminal action.

It is easy to see that the collection of all possible strategies for a given problem, even a relatively simple one such as the one we are considering, is extremely large. It is also clear that some strategies are inferior to others regardless of the true state of nature, and are not worthy of consideration. For example strategy 2 above leads to exactly the same course of action as strategy 1 and costs 10 cents more. In such a case, strategy 2 is said to be dominated by strategy 1, and to be inadmissible.

In general, in comparing strategies we need some specific criteria, once the clearly inadmissible strategies have been eliminated. Let us first consider the preferences you might have for the three possible terminal actions, if you knew the bias in the coin. These preferences depend on the consequences of the outcomes for you, and these consequences may be different for different individuals. Recall that it is the day before payday and it is your last three dollars. If you live in a rooming house and need the money for dinner, its loss may mean that you go without dinner, while an additional three dollars may mean only that you dine on lobster rather than hamburger. On the other hand, if you live in a boardinghouse and your dinner is paid for, the loss of three dollars may not be catastrophic while with an additional three dollars, you may be able to see a play that is closing that evening and that you have long wanted to see.

Suppose that you are in the first situation, but you are reasonably venturesome. You are willing to take some risks, but in this case, you would like the odds to be in your favor. If you knew that the bias in the coin were such that the odds for heads were at least 2:1 (or the odds for heads were less than 1:2), you would like to bet tails. However, if the odds for heads are somewhere between 2:1 and 1:2, you prefer not to bet. Suppose you are willing to go even further and quantify the losses due to a wrong action on your part when the true odds for the biased coin are in the stated ranges. You might come up with a table such as the following:

Loss Table for Wrong Decisions

True odds (Heads : Tails)	Action		
	No bet	Bet heads	Bet tails
2:1 or greater	1	0	6
Between 1:2 and 2:1	0	2	2
1:2 or less	1	6	0

By statistical convention there is assumed to be a zero loss when the preferred decision is made; the positive numbers represent the relative weight attached to the importance of the particular wrong decision involved.

For each possible value of the true bias in the coin and for each possible strategy, we can now compute the probability of each of the terminal actions and the expected loss due to wrong decisions when the strategy is used. It may be desirable also to take the expected cost of experimentation into consideration.

For example, we can compute the expected loss for strategy 3 above using the given loss table. In this case, the bet is never refused, and the face selected is the one coming upon the experimental toss of the coin.

Expected Loss for Strategy 3

True odds (Heads : Tails)	Probability of betting heads	Probability of betting tails	Expected loss
4 : 1	.80	.20	1.2
3 : 1	.75	.25	1.5
2 : 1	.67	.33	2.0
1 : 1	.50	.50	2.0
1: 2	.33	.67	2.0
1: 3	.25	.75	1.5
1 : 4	.20	.80	1.2

At this stage, we have a means of comparing strategies for each of the possible true states of the bias in the coin. Such comparisons generally make it possible to eliminate inadmissible strategies. However, we are usually left not with a single strategy, but with a class of admissible strategies. It becomes necessary to set up additional criteria in order to select one from the class.

One such criterion is the minimax principle. That is, suppose we have found the class of admissible strategies. For each strategy, in the class, we examine the losses expected for the various possible states of nature and see what the maximum loss is. Then, among the strategies, we select the one for which this maximum loss is smallest.

Another criterion is called the Bayes' criterion, but its use involves an assumption which many statisticians are loath to make. In this case, it is assumed that there is a probability distribution over the states of nature, and that we know what it is. In one example, this would be the case if we knew that the coin could be constructed with only three possible biases, say, 2:1, 1:1, and 1:2 for heads to tails, and that each of these biase is equally likely. In such a situation, the risk of wrong decision for any strategy is a single number which represents the average of the losses for the various states of nature. The average is computed using the assumed probability distribution.

We have now discussed all of the basic elements of a statistical decision problem. In such a problem, we have

(1) A set of available actions. Our preference among these actions depends on

(2) The true state of nature which is generally unknown.

(3) We are able to quantify our preferences for the actions depending on the state of nature, and set up a loss table giving the weight we attach to a particular wrong decision when a specified state of nature is true.

To gain information on the unknown state of nature, we may perform

(4) An experiment which yields us

(5) A set of data.

(6) A strategy for decision making is a complete prescription which specifies the experiment to be performed and the terminal decision to be made for each set of data which may be collected.

For each state of nature, with the use of the loss table, we can find

(7) The expected loss for any strategy.

If we are willing to specify the likelihood of the various states of nature, we can find a

(8) Bayes' solution for the problem, which minimizes the expected risk of wrong decisions over the states of nature. Otherwise, we may adopt

(9) The minimax principle in selecting a strategy.

In other cases, we might adopt a strategy for which the maximum expected loss seems bearable to us.

It is clear, I think that there are several inherent difficulties in the application of this theory. One is the need to quantify the various losses. Another is the number of comparisons which must be made if the number of strategies is large. Nevertheless, in making decisions for example, to change jobs, to go on to school, etc., it is useful to try to evaluate preferences for different actions, depending on the true state of affairs, and to think in terms of possible strategies. These strategies should prescribe for us how to obtain the information needed in reaching a decision and what to do with the information when we obtain it.

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Summary of Discussion with Dr. Sitgreaves and Participants

Question:

Where do we get the information on which to base a strategy?

Answer:

We shall need to collect more statistical information, as we have in the prediction of success in college on the basis of admissions data. Sometimes it is necessary to perform new experiments in order to get the information.

Question:

Is statistical decision theory always based upon losses, not on gains?

Answer:

In statistical decision theory, we formulate a loss function; economic decision theorists talk about "utilities," and utilities are positive.

Question:

Can utilities always be scaled on a single continuum?

Answer:

It is sometimes possible to reduce several scales to one, which may be dollars. In other cases this is not possible, but utilities can, nevertheless, be ordered, which is an approach to a metric.

Suggestion by participant:

It is possible that for any set of courses of action one criterion may be increasing while another is decreasing. So there might be one judicious place where the negative effects of the decision could be minimized and the affirmative effects maximized.

Answer:

In some cases one can more or less order the alternatives to locate an optimal decision. Then one can look at other decisions which get further away from the optimum, and presumably the further away the outcomes the greater the losses are.

Question:

Are you aware of any practical applications of decision theory to one-to-one counseling?

Answer:

I don't know of any. The principle undoubtedly operates, but the counseling process is seldom, if ever, formulated in terms of statistical decision theory.

Suggestion by participant:

Some people actually have to be taught to take risks. Risk-taking is for them an activity that is valuable for its own sake.

Suggestion by participant:

Middle-class clients who have had normal schooling through high school have the experience of logical decision-making processes even though they don't describe them in formal statistical terms. Clients from welfare situations probably have never learned to think in such a logical fashion, to say nothing of using such formal terms to describe their thoughts.

Suggestion by participant:

It is necessary to try to understand the client's real utilities, values, and goals. These may be very different from what he tells the counselor they are, and they may not be what would seem rational in the situation to the counselor. On the other hand, it may be that the client's decisions are really quite rational but his sense of values may be so different from the counselor's that his behavior appears illogical.

Suggestion by participant:

The value of decision theory may not be in its direct application to the client's problems. Rather, it may be helpful in providing the counselor with the framework. He can express the client's problems to himself in decision theory terms, then listen to the client's exposition of the same problems. Where there are striking discrepancies the counselor can seek to understand why.

Suggestions from participants:

A great deal of counseling is what might be called pre-decision counseling. When the client first comes in he may not be ready to use any logical

processes in decision-making. A lot of irrational counseling must go on first. "Until this person gets up to the point where he can say, 'I can choose whether I want to be this way or not be this way' not too much has happened. An awful lot has to take place prior to this... and perhaps this is when there are a lot of irrational pluses and minuses along the line. Then the client finally reaches the point of saying, 'I, John Doe, now have a choice. Do I want to continue to live this way or do I want to change this pattern a little?' Then the counselor can start to look at the alternatives in both directions. Until this happens one cannot be sure whether the person is going to leave any better off than when he came in, or more frustrated."

DISCUSSION IV

Drs. Abraham H. Maslow and Rosedith Sitgreaves and Participants

Hopf:

It seems to me that in the structured interview we may be seeing peak experiences in the counselee, where suddenly she sees a unity or suddenly she realizes what her experience in the past means in terms of her personality and her potential. But my peak experience in the interview is quite a different one. It is a reinforcement of my satisfaction with the job that I have chosen: how it fits with my personality and how it fits with my whole life.

Maslow:

Yes, that sounds familiar to me. I think that the most modest thing to say is, that that is out on the edge of knowledge. You are in the difficult situation of comparing inner psyches. It is quite difficult, but what you say sounds familiar. I think that the kind of satisfaction that you get, the small peaks -- the "foothill experiences" I called them once -- that you would get out of your work going along well, are the rewards that keep you at your work. I think that this would be somewhat different from the youngster or the patient, because while this may be the first time in his life that he has ever experienced anything like that, you have presumably, been experiencing, and you know what to expect. It would certainly be less intense, I would guess, more expected, less sudden. We just don't know enough about this. So far as I know, I think my descriptions are the first that are entirely naturalistic. In the past couple of years some of my students and friends have been doing this. Of course it is still at the level of "chat" rather than "careful report." It will take another decade for this to get really organized, I think.

It is possible, if I may say so, for those of you who are interested, to do careful research. I did once, not to teach myself anything great, but just to demonstrate that it was possible to manipulate experimentally, statistically, these data. We asked our students at Brandeis to submit a questionnaire, reporting their peak experiences. Then my wife and I went through perhaps a thousand of these and tried to classify the triggers which set them off. We found some very interesting things. Now one statistically significant difference we found, which I worked through just to show that you can talk that language in this subjective realm, is that our young men reported significantly more often than women, peak experiences from experiences of conquest, of victory, of achievement, and success -- as common knowledge would dictate. The girls significantly more often than men reported peak experiences from being loved. Well, that's it. It fits with our common knowledge and it demonstrates that you can treat such subjective data with our normal statistical techniques.

Meyer:

Was there a parallel difference in the male in loving and being loved?

Maslow:

The males more frequently reported peak experiences from the sexual experience than the women did, although that was not significant as I remember. It approached significance. Characteristically, if I can think back now, the women reported being loved as a great moment in their lives. Some He said to some Her, "I love you very much," or they got engaged or something like that. No man reported getting engaged as a peak experience. (Laughter)

At Brandeis University, a very fine school with a very high selection, (it is probably harder to get into Brandeis than any other school in the country in terms of IQ, etc.,) not one single student out of a thousand reported a peak experience from an intellectual experience -- not one! Now for me this is a fascinating thing for research. You can manipulate it, it is not just subjective. I am a scientist, you know. Just sort of free-wheeling about what I will use and what I will not use, but clearly I am not reporting my hallucinations or my own wishes or anything like that. I am reporting objective data which somebody else can check on.

Lichterman:

In this connection, may I ask if this ties in to some of the things that Bernice Neugarten had to say. Do you find any significant classification of peak experiences? You mentioned that there is in terms of sex but what about in terms of age, or somato type?

Maslow:

There I have only the suspicions which I would like to check. If I judge by my own experiences to start with, and then with a few friends, it looks as though certain kinds of peak experiences -- triggers -- go down with age, others come along. As a matter of fact, peak experiences in general seem to go down with age, that is, "the great explosion of being" moment, to be replaced by something else which we don't have names for yet: serenity, perhaps; quiet cognitions of various kinds. But the major sources of peak experiences in the world are missing. When I first discovered music I was in a state of tremor all the time. I can remember, as if with a sweetheart, the first time I heard the Firebird Suite, the first time Brahms' First Symphony got through to me -- it would tear you apart! And now I have heard the whole standard repertoire you see, 2,000 times, I can't even hear them any more. Well, that's lost! Many things are lost as you get older.

Participant:

You use the example quite frequently of the aesthetic experience as a peak experience. I wonder if you would like to comment on this. I remember the story of a woman who had heard Fritz Kreisler in one particular piece, ... who apparently had a peak experience in hearing him play this particular work. Afterwards she went up to him and asked, "What were you thinking about? I got such a tremendous reaction! What were you thinking about?" He said, "I was trying to think about catching a plane and getting out of here." Now there doesn't seem to be necessarily any one-to-one relationship.

Woditsch:

I am curious to know if there is any particular way that you might articulate the decision-making process, as expressed in Dr. Sitgreaves' notions with your thinking. You take your great people, those whom you have used to form your notions of self-actualization. From your discussion yesterday I get the impression the decision-making process is a matter of finding your own internal resources, being alerted, sensitized to your own conditions, needs and wants. Dr. Sitgreaves, unless I am mistaken, seemed to indicate more of an environmental concern, estimation based on whatever we can gain of knowledge about our environment, thinking of the application of fairly articulate principles to that. What would one of your great people be characterized by doing in the decision-making process?

Maslow:

Let's just turn that around a little bit and discuss some of the questions that I wanted to bring up with Dr. Sitgreaves last night, and then we can come back. It raises the ultimate questions about epistemology and about the nature of knowledge in general. Our western tradition of science, of rationality -- not just science but rationality in general -- has been to reject what another age took very seriously as a source of knowledge: the whole of the mystical experience and mystical knowledge. What happened to mystical knowledge? Yet now I want to bring it back! Raise the question of validity as a source of knowledge, of the kind of very profound inner-certainty deciseveness, of perceptiveness of kinds of reality. I am writing a book and I don't know what I will call it exactly, Psychology of Science or something, certainly one aspect of it is -- but questioning this whole epistemology that we picked up, with reality being handled in certain ways. How can we test the validity, the veridicality, of the "being" knowledge I call it -- mystical knowledge it is called also. If I can make it even sharper, let me go to the pathological extreme if you don't mind. In your most obsessional and compulsive people, who would be seen by the psychodynamic people as not having a strong identity, not having a firm self, not really knowing what is going on inside of them, the ones who look to calendars, and who collect train schedules, who have everything scheduled on the moment, and eat prunes every Wednesday morning for breakfast, and so on -- rather sad people -- they don't know when to urinate, they urinate at a particular

time; they don't know whether they are hungry or not, they hear no inner voices. Such people at this pathological extreme turn to the outside world for cues as to what is right and what is wrong. Cues about decisions. As you go in toward the more normal center, the question would come finally for admittedly healthy people in making decisions, about the relationship between cues from the outside and cues from the inside. Now for instance, one of the questions I wanted to bring up last night is out of my own experience (one that the statisticians so far as I know have not discussed but I think they must because it is a problem that will have to be handled). It concerns the goodness of the chooser, the goodness of the decider, and this brings us into the whole realm of psychological health, for instance. The person with the strong identity, the person who knows when he is hungry, and who can't be fooled in the Asch experiments, the non-conformer, the resister, the one to whom you cannot suggest things, who knows what he knows, the self-actualizing person who generally knows what is right and what is wrong, may get confused intellectually if you talk with him, round-twist him, and yet he just feels about it a certain way. He won't do it, he can't rationalize it, it is just wrong for him.

Well, if you don't mind, I think my own experience is typical. I have now a long lifetime with researches of various kinds. These have slowly changed and become more daring. When I was a younger man it was standard laboratory work. I was doing animal psychology, learning psychology. As I got older I learned that I was a good guesser. And no single experiment that I have ever done has ever been contraverted. Everything has been replicated. It always comes out on the nose, even as my sort of intuitions, things that I was doubtful about like this self-actualization business which I didn't publish for seven years. So that now at the age of 57 by comparison to the age of 37 -- this is an empirical statement -- I can go back over my work, my experiments, bibliography, things I have published, and say: "Thirty times I have stuck my neck out and said something must be so; thirty times it was so." Now I start getting grandiose.

I take my intuitions very seriously, and then at this point I think of the mathematicians, the great ones. The mathematicians more than any other people make this distinction about intuitive certainty, when you look within yourself. A mathematical genius is a guy who knows what is right and then spends four years trying to find out why or how. And this is the difference between the very good mathematician and the born mathematician. It is intuition of this sort. He is a good guesser. He achieves the right answer and he can't tell you why, he doesn't know. Well this is the kind of thing I wanted to discuss last night.

Another one was, by the way, something I wanted to ask about the emotional side of statistical work. This can be, as you well know, a very beautiful thing. And there can be great moments in this as the last figures show up, after the two years of work come to a climax. It is almost a sexual experience. Well what I would like to raise is why the statisticians and mathematicians comb out of all their public descriptions all the things that secretly, privately, they love their work for. That is why they do their work.

These are the two questions: first, about the nature of the chooser and the nature of his decision process and the fact that some people trust themselves more than others and that this itself is an empirical matter. You can learn from 30 years of experience as I have. You discover whether you are a good chooser or not. I can say calmly, and I don't know whether this will make your hair stand on end or not, I can more or less judge my clinical competence in terms of 300 people that I can go back over -- how right was I, how wrong was I, how often -- for intellectual competence, I can judge that. I know, for instance, my judgment of intellectual competence is very close to perfect if I have the right conditions. My clinical competence is less so, and I know how much less so. That in itself is a statistical datum. These are a lot of questions.

Sitgreaves:

There are several things that I would like to say. One is that within the last maybe ten years there has been work carried on by Jimmy Savage and others on subjective probabilities and the fact that when people make decisions, whether they think of them in this way or not, they are using certain probabilities...

Maslow:

You're using that as a technical phrase?

Sitgreaves:

Yes, that's right. Yes, "subjective probability" is a technical phrase. Just as we are now working in behavioral sciences very much the way in the past they worked in the physical and natural sciences, we are now beginning to try to measure -- as everybody in counseling uses the Strong and all these things that are supposed to measure vocational preferences -- things that 50 years ago people would have said it was impossible to measure. They laughed at Binet, you know, when he talked about measuring intelligence. Well, there has been this development in the area of subjective probability and how people make decisions using these probabilities. In addition, there has been some theoretical development by Herbert Robbins, in what are called empirical Bayes solutions, and this is exactly the kind of thing we have been talking about. As you go along you collect experiences. You collect essentially data on the correctness or the incorrectness of certain decisions you have made. Herbert Robbins is co-author with Courant of this book, What is Mathematics?, but he has in recent years done some work on what he calls empirical Bayes solutions. You know Bayes -- he was a very early statistician who was a minister I believe, who propounded this theory: -- you kind of work back from what you observe to what must have been the state of nature that produced it. For a long time he was in disrepute. In fact when Wald first started formulating decision-theory, he brought up Bayes' solutions, but he was always careful to say that he was doing them simply as a device. Well again within the past ten years in statistics and in decision-theory, a number of them have

gone back to this notion of a priori probabilities for various states of nature. Again going back to the notion of subjective probabilities so that you believe that certain things are more likely to be true.

Maslow:

You mean on the basis of actual experience?

Sitgreaves:

Yes, or for some psychological reasons, but generally on the basis of your past experience. If every time you crossed the street there wasn't any traffic, then at some point you may cross the street and be hit because you are just assuming that this kind of statistical regularity will continue. So there is gradually more being done looking at the way people make decisions, trying to see whether or not they can verify these notions of subjective probability and there is some evidence now, I think, that people do operate in this way.

Maslow:

I had one example here to indicate what you have said, just to indicate the kinds ot techniques that you might very well work out. There was a joke that I quoted someplace that I heard from somebody and had to pass it on because it was wonderful -- about the man who had made the same mistake for 30 years and called it rich clinical experience. (Laughter) This is the question of sound testing. That is, of discovering one's own clinical acumen, plus actually testing yourself to see if you are right. What I have done is to keep records. And now I make my guess, by gosh, write it down, record how it works out. I know my mistakes, and I know how often I have made them, and so on. Is this not a very promising approach to the whole question of the goodness of the chooser, of clinical acumen, and so on? There is no new mathematics required to handle this. I would think it a simple matter.

Siegle:

I'm terribly worried about this conversation. What I get out of all this is that all the peak experiences that happened last night have gone down the drain now because it is no longer statistical. The subjective probabilities are about the same as they were for me, you see. I can't deal with the whole universe. And I don't see where I have gained anything with this conversation. I've gone back about fifteen steps.

Maslow:

You have missed the fusion. I think what I was suggesting was that these two worlds are not really very far apart. They can be melded right into each other.

Siegle:

Yes, but why bother melding them, is what bothers me now, why bother melding them? You're in good shape and she is in good shape. I don't know why you want to get so close together -- these two worlds.

Maslow:

Well, I think I have a counter there. If you were to read the history of mysticism you would see why there are very, very sound reasons. The Renaissance, the new scientists, they rejected the whole blasted business because it is unverifiable, nuts can get into it. You can't tell the difference between a valid mystical experience and the cognition and the mystical experience of a schizophrenic. You need external verification. It cannot stand on its own feet, except subjectively. It always needs to be tested to find out, was it really so or not? And how do you know if it is really so or not? Subjective certainly doesn't do the trick alone. It just doesn't. From the subjective feeling an incorrect illumination feels exactly like a correct one.

Siegle:

Yes, maybe my density here is great but...the factor of the subjective probability...I don't see why you therefore accumulate all the subjective experiences and put them into some sort of probability relationship. This doesn't perhaps compound the varieties of schizophrenia. I want help from you on the validation. You are validating to me the validations of a schizophrenic by a schizophrenic. I want to get out of this. It seems to me not so much how close the two are, but how Rosedith's work can really help validate it. It may have been there, but I didn't get it.

Sitgreaves:

We are in the tradition of wanting to know all about people, all about life, of what is happening, you know, what's going on, and the scientific method presumably says that we can really be sure of our knowledge only if we can replicate what we have done, if we have objective measurements which are apart from subjective bias. Now there are two things: it seems to me you can never get away from subjective bias. One of the things I was interested in when I first started teaching statistics was -- I had thought of the great rigor of science, with some immutable facts -- that I found there were certain text books that I just couldn't stand because of their point of view. It wasn't the facts, it was the point of view that was completely different from my point of view. And so no matter what you are doing or how rigorous and objective you try to be, there are certain subjective elements that enter it, and it seems to me that maybe this is a good thing. It would be awful if everything could be reduced to numbers, you know. But the other thing is that statistics, as such, really provides only a set of methods for handling information. We could talk about subjective probability but this is of interest only if it throws light on the

way in which people actually operate. If we do have statistical methods that we use at school for handling the information that we get -- you know someone said last night and it is very true, that people, at least in some articles in the literature, seem to confuse statistical significance with meaning. Statistical methods as such really don't say that anything is good or bad or worthwhile. They are purely tools. One of the unfortunate things, I think, is when the tools get in the way of the knowledge. They are simply like a saw and a hammer and something else that you use in constructing, I don't want to say a set of facts because it seems to me it is much more than facts. We are trying to develop knowledge, we are trying to learn how people behave -- I think to some extent we are both helped and hampered by the fact that the behavioral sciences are trying to become sciences, and, as such, are trying to become more mathematized. I mean even the formulation of statistical decision-theory was essentially a formulation of a mathematical way a statistician behaves or should behave. It simply says, there are strategies. We need some basis on which to choose good strategies from poor strategies. Partly because a great many people don't have strong intuitions. I was thinking when you were talking, that in statistics, of course, the great intuitive person was R. A. Fisher. If you look at the statistical literature, the references, you will see -- "First proposed by R. A. Fisher later rigorously proved by somebody else" -- time after time after time. With the Neyman-Pearson theory, when they tried to talk to Fisher about it, and about how did he know which test was most powerful, he said that he never had any trouble deciding, and he never did.

It's a little bit like playing bridge. There are great natural bridge players, but there are other people, especially when they are learning the game, who need the point-count system and need to know something about rules for bidding, rules for playing, until they, by experience, acquire the kind of background that makes it possible for them to handle the playing situation. It seems to me that this is true in statistics. After a while you no longer feel that it is necessary to do lots of statistical tests. You are willing to look at the data and make some judgments as to whether or not the two distributions are really different. I am very much against preliminary tests myself. I would rather say, "Well, you know the variance seems to be relatively homogeneous." I am willing to put confidence in my own judgment here in looking at things like this, but I suppose until you have acquired experience, until you have got some feeling for the way numbers behave, you do need tests. It is very easy to do a pilot study with 20 people. This happens to our doctoral students more times than we would like. They do pilot studies and get beautiful differences in the pilot study when they are using 10 people in each group, and they go out and they study 40 or 50 people and the differences just wash out. So that there is this fact that differences for small samples don't necessarily hold up when you take large groups.

Actually I believe in statistics. We need statistical techniques to help us evaluate this kind of sampling effect, the sampling and stability. This is all that tests of significance do. The thing that always makes me feel happy

about statistics is not the differences, it is the fact that when you get large groups there is statistical regularity. I was interested several years ago when they were trying to evaluate the three biology text books. The biologists got together at Boulder to rewrite the biology text book for the 10th grade and they found that they disagreed so much on the point of view from which the text book should be written that they ended up with three versions - I think the green, and the yellow, and the blue. At the end of the year or at the end of two years, well, they used standard tests and there was some argument about whether the standard test was really testing the effects of the differences, but in any case, they used standard tests. They had different publishers involved, and everyone was sitting around with bated breath to see which was going to be the better one, and the mean scores for the three text books, plus a conventional one, were so close you didn't need any kind of statistical test to show you that there was no difference. This was a null hypothesis which you could accept with complete confidence because the means were something like 83.2, 83.5, and 82.9. You know, this is what gives me faith in statistics. There is a regularity.

Meyer:

There is a direct relevance here in the problem of significance and meaning to the individual counselor who has before him a set of test data on a client, and while there may be a great statistical or empirical significance to much of the test data, it won't have any meaning at all until the counselor incorporates it into the counseling process and until the client and he can get together and decide what this really means for this particular client. And I think too often we tend to look -- you know. It's like the gospel of the printed word. It is down on paper. It must have meaning -- positive or negative -- but it must have meaning.

Maslow:

I have a phrasing here that I think says what both of us have been trying to say: the statistics are not the substitute for intuitive decision but they are an extremely useful tool in the hands of someone who knows how and when to use them. Furthermore, in contrast to computers, if I remember, the one thing that I felt was wrong with computers that hasn't been discussed sufficiently, they are too darn consistent -- they never know when to make an exception.

Sitgreaves:

And they are so stupid!

Hopf:

When there are too many right parentheses, they don't know that they should just lop off the extra right parentheses.

Maslow:

Last night we were talking about an example of that kind of thing that I suffered through... it took me 5 years to get my BA because I moved from school to school, and each time I accumulated new required courses. I wound up at the University of Wisconsin, and I was teaching on the faculty and required to take freshman composition, freshman physics, freshman German -- four freshmen courses. Some guy there with facts in front of him, feeling perfectly virtuous there, with the facts -- obviously I was short one-half credit in something or other, and he couldn't even see, didn't even think it was funny. For my first theme in this freshmen composition course I brought in a published paper. (Laughter) Well, he was a computer, that is what he was.

Grenfell:

We are interested in change, and I wonder if there is some work being done in terms of statistics whereby we could encourage the counselee to sample his or her own behavior and make predictions in terms of it as to what the outcomes may be. Then since he has some kind of a formula to work with, make a conscious effort to change so that the outcomes would be a little better for him. I am thinking in terms of a mother and child interaction. To much of what you do, this child had undesirable reactions. But the mother has been reacting in this way for so long it has become a habit, so she is unaware of it, like the person crossing the street. Could it be more meaningful for this woman if she sampled her own behavior for herself rather than taking the behavior of the population of 500 other mothers?

Sitgreaves:

There is some work being done now in formulating procedures for handling data that are collected on an individual over time, multiple measurements for the individual. There has been some work... well this is not altogether new. Most of the trouble is that the work that has been done in the past, is not, it seems to me, particularly readable. There is this work by Stevenson on the "Q" technique where what he proposes has never really been very well investigated statistically. He claims certain properties, but it is not clear these properties are actually there. In general, people are now much more interested in the dynamic processes, in time processes, and are recognizing the fact that statements about a group characterizing the group, are not particularly meaningful when you come to the individual. You are much more interested in samples of behavior for the individual collected over a period of time. Now there are theoretical models that are being set up for some of these time processes. The Markoff model is one of them. That is where you have a number of states, the individual can be in any one of the states at a given point in time, and essentially you are interested in the transition probabilities of going from one state to another; so the system is one state now and another state possibly tomorrow. But if you can think in terms of the state that you would like to move to... the state which would be preferred... how do you go from one state

to the other? The other thing I think is, (and I think that most people in practice do this, although again it is not always clear in the research) the great tendency to look at one variable at a time rather than to try to look at the way they are interacting. For instance, I was thinking in terms of decision problems. A college admission officer in connection with each student has only one of two decisions to make -- to admit him or not to admit him. Now what is he going to base the decision on? Well he is not going to base it on any single variable. Certainly he will look at SAT scores, he will look at all of the preliminary tests, he will look at the high school record, but usually there are three situations: there are the clear-cut cases of the people you certainly want to admit, people you don't want to admit, but the problem always comes with the people in the nebulous region who might be admitted and you have to reach a decision. Presumably, as I understand it, in some cases they do actually have a formula of least weights, there is something like maybe 10 things they look at and they have different ways of combining them. They specify a strategy for admitting people to college, but it is not based on any single variable. It is based on combinations or on patterns.

Southworth:

Isn't the high school rank a summary statement itself?

Sitgreaves:

Well, yes. The high school grades are the best single predictor because presumably people take into account some of the psychological variables. You know, drive for achievement, etc., which are not always measured by the test.

Wientge:

About this question of peak experiences: is it true that if we could collect a universe of peak experiences from many, many individuals, analyze them and quantify them, we could somehow add them to our predictive instruments just the way we do high school grades?

Maslow:

I don't see why not. Since even different kinds of constitutions, the mesomorphs and the endomorphs, and certainly the males and the females, trigger off for different things. It might conceivably be that if you knew enough about the peak experiences of young people, we could see where they were going. That is, what they really loved, not what their Daddy thought they loved. Part of some future development in, let's say, vocational guidance, would be to find out what they were born to do -- what their bodies reacted to -- what gave them zest and happiness and joy! But I am just impatient to add this: I have been trying very hard to make this fusion which I think is so important. I have to do this because I get classified. If I am trying to resuscitate mystical knowledge, well, you can imagine what my colleagues think of me. (Laughter)

And yet I would point out on the other hand... I think I know the dangers of taking the mystical experiences too seriously. If you read the histories of any religious group, it always winds up in some kind of magic, and so on, so it has to be done cautiously. Yet the fact remains that we must remain with a good chooser. We must develop a science of a good chooser -- how to create him, how to educate him, how to make our youngsters better choosers. We are planning an experiment at Brandeis. It is at the "white-rat level" but I hope it will come along soon. Simply finding that among any species of animals so far including the human being, it looks as if you get constitutionally good choosers and bad choosers. Now we may do this either with monkeys or with white rats but I can demonstrate that animals left in the cafeteria situation, you know this one about choosing for themselves -- the wisdom of the body kind of thing. If you put animals, any species I can tell you, in that situation, some of them are going to die. They are just lousy choosers. They don't know -- their bodies don't tell them what is the right thing to do. Their taste is not a good cue as to what is wise for them. That is already reported for human infants. Gesell reported it, and other people have. There are some babies -- you leave them to their own resources about feeding themselves or when to stop eating, and they don't do well -- a small proportion, we don't know how many. Well, what I have suggested is self-examination, self-scrutiny, self-testing, so to speak. That is, you ought to accumulate records about your own experience and discover just how much to trust yourself and in which areas -- when yes, and when no, when you go astray, and when you don't go astray. This is self-knowledge that we people in all the helping professions ought to have. Some kind of index for ourselves. But in addition to this I think we must use this idea to pick out the guidance man. We should pick the spontaneous good chooser, the one who has intuition, the one who is wise already, who has good judgment and good taste. Then the tests and the norms and the methodological tools, the statistics and so on -- we can trust them to him. Obviously all the tests and all the statistical tools that we have are useless in the hands of a jerk. (Laughter) Isn't that true?

O'Hern:

Do you feel that a self-actualized person is the one that has this ability?

Maslow:

Yes, I reported them first. I didn't think I reported them as having some kind of built-in system of ethics. What I reported behaviorally was that they seem to be sure of their judgments of right and wrong, they don't get confused about things. That's what I reported. Now from that it goes over into all sorts of other things. You ask them would they like some sherry, or wouldn't they like some sherry, and they say, "No, I don't like sherry." They know they don't like it, and they seem to be more decisive; these are decisive people.

O'Hern:

May I come back to the gray area here in terms of admission to college? I hear you saying, if I have the ability or if I am a good chooser, I can do just as well, maybe even better, than the admissions officer does statistically. I wonder if maybe it's because you are adding other variables, that we haven't been able to quantify.

Maslow:

I guess I just haven't been able to make myself clear. What I was saying is that if I regard myself on the one hand, I have been talking about intuition, good choosing, trusting when to trust your impulses. On the other hand I would like to speak as a statistician if this doesn't surprise you (I have published contributions to the statistical literature -- these things are not incompatible with each other). To talk about mystical illumination on the one hand and to talk then on the other hand about refining, proving, and making more certain. Actually a good chooser can be helped, he can become better, he can be given all the technical tools that we have. Between the intuition of a good chooser and a bad chooser, of course I would choose the good chooser, but as between the intuitions of the good chooser, and intuition plus scientific work of a good chooser, the choice is obvious. He can improve with all the paraphernalia of science, empiricism, all the methodologies that we have, including all the statistical methods. These are ways of improving and not replacing the sensible human being.

O'Hern:

I guess I have to say that I hear what you are saying, but I also hear you one step beyond.

Bryant:

Would you say that the person who is congruent, who brings a wholeness to this choosing situation is more likely to have sound judgment, rather than a fellow who says on the one hand this, and on the other hand that, and he is sort of lost?

Maslow:

This is a tricky business because the most decisive people we have are paranoid people. There you must have an external criterion to go by. But if your choosing people were more trained, then by picking the ones who were most talented, the most promising prospects for training going through the study for four years, six years, the best ones to pick out for that kind of training are the ones who seem to be good choosers.

Sostek:

Does this seem to be an inherited, a constitutional quality?

Maslow:

It is quite clear from the animal studies that it is partially inherited.

Sostek:

Let me ask you a question then. Does a good guess, I mean a guess like you said -- 30 shots, 30 bull's eyes -- does this essentially, at the completion, lead to a peak experience? If a statistician works two years on a hunch and comes out just right, is this essentially going to lead to peak experiences?

Maslow:

It may. Nothing does with certainty. This is one source of peak experiences -- to turn out right guesses.

Sostek:

Then the other question: can we train people to be better guessers?

Maslow:

I think so. That is what education should be for because when I did this work with human beings if I got the real -- if I may use this word -- the "Schlemiel," the ones who always choose wrong, (laughter) they always choose the chair that collapses under them. Well these are the ones you cannot train.

Sostek:

This is the "woe is me" person -- "nothing ever goes right with me."

Maslow:

They are the lousy choosers.

Hopf:

No, no! They are choosing punishment aren't they?

Maslow:

I don't think so. That's Freudianizing. I have worked with such people. Sometimes it is a matter of self-punishment, sometimes clearly not. There is a kind of stupidity we haven't yet described.

Sostek:

If this process is partially inherited, we have got to learn to talk about the developmental process. I believe we even talked about the question of who has the dopes for parents. Couldn't we, in learning to choose, improve this guessing process early in life -- you know, "don't do that, do this" ...?

Maslow:

I don't know. I think I don't even have anything to guess on there. I know that in general it is easier to destroy human beings than to make them; to destroy capacities. But on the other hand, we do accept the fact of genius somehow, you know, the Mozart kind of thing, or the two areas where it has been demonstrated most -- in mathematics and music -- the kind of inborn genius. If I set you the task of producing me a couple of dozen Mozarts, you wouldn't take it on. But if you have people who already are talented, I think I can. I do pick for, let's say, graduate studies in psychology. I try very hard, we all do, to pick the ones who are born psychologists, who have a knack for it, who are talented. Well, it is hard to put this into words but I think we all do that in any admissions committee of any kind. It is the people who have the potentialities, who already are good, these are the ones we pick to perfect, to train, to teach; and we feel that there are some people who simply don't have a talent for psychology, and if I can catch it, I will say "This is very foolish." You couldn't train them in 40 years except to be third-class psychologists.

Sostek:

Are you in any way relating successful therapy with enhancing and improving their ability to guess more correctly than before?

Maslow:

Yes. Not always but...

Sostek:

Maybe if we could get people guessing better and to have better "guessing training," for want of a better expression...

Maslow:

Successful therapy should improve one's choices. Put it that so many youngsters, for instance, don't pick good wives and good husbands for themselves. It is a matter of their being poor choosers, and this happens to be repetitive, Freudian usually -- their needing something unconscious, and looking for it. Like one of my subjects once; a woman who married three men in a row who turned out to be homosexuals. Well, psychotherapy could help her.

Sostek:

Yes, I mean this thing, it hangs on. She doesn't change her guesswork on the basis of past experience.

Maslow:

Yes, in psychotherapy she perceives more clearly, percieves reality more clearly. This is the usual appraising of psychotherapy.

Senders:

Doesn't our training in psychology sometimes take these people who have the ability to be good guessers and very rigorously train it out of them? People select themselves to go into psychology, some of them, partly because of, or in spite of all their personal difficulties and so on. These very personal difficulties and maladjustments that will take them into psychology may be behind their ability at some good guessing. And then we subject them to learning theory and statistics, (and not the best kind of statistics -- the poor kind) and the rigors of psycho-physics. We teach them to distrust. I would like to read into the record a contribution from George Santayana. I have been debating as to whether it would be appropriate to do this and I think it would. Bear with me: Santayana said:

It is not wisdom to be wholly wise
And on the inward vision close the eyes,
But it is wisdom to believe the heart;
Columbus found a world and had no chart
Save one that faith deciphered in the skies.
To trust his soul's invincible surmise
Was all his science and his only art.

Our knowledge is a torch of smokey pine
That lights the pathway but one step ahead,
Across a void of mystery and dread.
Bid, then, the tender light of faith to shine,
By which alone the mortal heart is led
Unto the thinking of the thought divine.

Maslow:

And to this we must add the footnote that Columbus was not a schlemiel! (Laughter). When people come and ask me, in the advising situation, trusting my judgment, trusting the inner voice and so on, I have to make a quick decision about these people. Or when it comes to mothers and children, I'll take a quick look up and down -- I have to, running between classes or something of the sort. It's a worried mother, and she looks to me to be levelheaded and sensible, healthy enough, and so on -- and then my advice: "Don't read any

books on child psychology; don't consult anybody; don't ask anybody. Every time you're in doubt, close your eyes, look within yourself and what it says there, do that!" Then, in the next hour, somebody comes along and I look her up and down, and there's clearly a rigid, extremely neurotic woman, and I say: "You'd better see a child psychologist: you cannot trust your own judgment; your own judgment is no damn good." I don't say it that way, but in effect, that's what we have to say.

Bryant:

Is there an analogy with the person who has a lack of balance and wants to step out with both feet at the same time, and falls on his face?

Maslow:

Very peculiarly there was an experiment done a long time ago on mixed laterality. This breaks up the confidence of a person in himself -- if your right eye and left hand are dominant. I can think, in working with developing children in the elementary grades, of the child who makes a very good physical choice, vs. another one who is just awfully crude at that kind but, is ethically pure.

Participant:

Can you take the physical good chooser and help him to develop his sense of ethics?

Maslow:

I don't really know, but just from general therapeutic experience, if you take Yogas, trying to deal with that question about what's natural to you, it is suggested that you develop your shadow-side to develop what's not well developed, as much as you can, without losing what is already your talent -- strategy and tactics rather than simple, straight yes or no. I guess we'll have to learn if people are different; the fact of individual differences really hasn't struck through to psychologists yet.

Meyer:

This is an amazing statement in 1965 and I certainly agree. I think that one of the things we keep saying over and over again is that while we talk about developing potential within the individual, actually what we're doing is developing those potentials we decide are good or bad potentials. This is an extremely important point.

Maslow:

You know all that sociological work on middle class values, for instance, something I was talking about last night. Or the relationship between men and women.

I'd be very radical about that. I'd say there are very few men who really like women, and there are very few women who really like men, in the ultimate sense of accepting them as they are and approving of them. It's nice that they should be that way! The women marry them and then try to domesticate them, which is like trying to feminize them, and the men push women out of their lives altogether and say they're nutty, "women are all nuts" and so on... One of the things about science -- I'm saying it quite formally in this thing I'm writing -- what you get in good scientists, I think you get in good people, too. One of the effects of great knowledge is to enhance that feeling of mystery. I don't know, we talk about prediction and control and so on, but to stand in awe before what you're studying is, I think, the mark of the good anything, and the more he knows, then the more awe he gets about how little he knows, and so on. I was trying to say about these individual differences -- the male and the female, the athletic and the intellectual, and so on -- for the love of Pete, enjoy the mystery! Sure, women are mysterious to men and I suppose always will be, but you can't be the other. But it's really such a pleasant mystery when you come to think of it. In the same way that it's a wonderful thing -- let's say that there are engineers in the world. I couldn't be an engineer -- if there weren't engineers in the world think of all the distasteful jobs I would have to do" So I am so happy they are there, people who love those jobs -- and we supplement each other and this is a wonderful thing. If it really strikes through to you in that way, then it's possible not to have to mold people into shapes that you can understand or that you are comfortable with, but with youngsters -- children -- I think this is very tangible. With average teachers, even average parents, you try to mold these kinds into preordained forms of some kind and this is what I mean by the rejection of individual differences.

Meyer:

Every class of teachers that I've ever taught has always, over and over again, stressed the point that the purpose of education is to inculcate values, and they firmly believe this, and when you're trying to break them out of this for guidance purposes and for teaching and for life purposes, you run into a helluva problem.

Maslow:

Well, the easy misunderstanding there, which is very widespread, is that if you think in terms of a real self -- if some place inside of me there is a bent, a natural tendency of some kind -- then my findings so far are both species-wise and idiosyncratic; that is, there are things within me that I have to discover for myself, and which I did. As I say, I was psychoanalyzed and discovered myself or rediscovered universal truths which are true about every human being -- what I was hurt by, what I loved, what I was afraid of, and so on. Well, these were lessons which I could have learned -- I wrote a book on abnormal psychology before I was psychoanalyzed and there are all sorts of statements there which are true about human beings. And then when I discovered them for myself, that was different, then they REALLY hit me. In addition to this,

in good depth therapy, you will find the ways in which not only are you common with all other people but the ways in which you are unique, different from all other people, so that when I discover this for myself and find out that, by God, I never will be an athlete (this was my heart's desire as a youngster). I am an intellectual, a book guy -- at first this meant the usual thing in psychotherapy. You give up an illusion, you give up a hope, an ideal, a social ideal -- well, I couldn't be a football player and for a young man this was hard to give up. But OK, so I'm stuck with it, with books. But then, I finally learned to love it and to be proud and pleased and happy with my fate. We were talking about that yesterday. But these would be the values which are right, which your own constitution dictates, values which are shared by everybody in the whole world also.

Southworth:

I have a question related to the training of counselors. You spoke yesterday about psychoanalysis and you were saying some very positive things about it, raising some questions about it at the same time. One of the things it seemed that you were saying is that we need to somehow get to a position of humility, and as good counselors we need to be very much aware of ourselves and our own feeling and of our sensitivities. Are there better paths to this than psychoanalysis? What are some of the evils of this? It seems to me, for example, that one aspect of psychoanalysis is that if you're talking about the classical situation, it's a road to humility almost through a humiliating experience. Maybe there are better paths than this.

Maslow:

Well, in the Bugenthal book I think I suggested, The Search for Authenticity, Jim Bugenthal was trained at various levels, and I would distinguish for myself the four levels of therapy, you might call it, as Jim has there. First of all, the social, prior to psychotherapy: starving people don't have neuroses, just feed them, simply taking care of the lowest needs. Then, beyond that, when they are taken care of, neuroses emerge; the classical neuroses coming out of lack of love, and respect, and safety, and they can be managed. Psychoanalysis is good for that -- there are good analysts and bad ones. The ideal psychoanalyst can manage that nicely and in this classical style of discovery that "you've been missing your mother who didn't love you" and so forth, and you discover what you like and what you need. But we know, now, also that that is not enough -- that you have to go beyond that. It is possible to be satisfied on all these basic needs, but then we come up to the questions of transcendence of meaning, of religious questions -- "who am I?" -- questions of identity in the direction of life and so on. You might call them the existential questions, and Bugenthal talks about their relationship between psychoanalytic things. He was trained as a psychoanalyst, but now he would call himself something else. He is an existential psychotherapist and has a new branch to be developed yet, a new kind of I don't know what -- the value therapist or counselor, mediator, or some darn thing, when we're at the level of our ultimate human values. I

mean we need to help ourselves. I don't know, maybe no one can help us there, maybe we have to do it for ourselves, but there are top levels. I think, for myself, psychoanalysis was very, very advantageous; I'm very grateful for it, and yet it has shortcomings. It goes just so far but it certainly doesn't go beyond that -- the existential psychologists have been adding things on. It's not pro-Freud or anti-Freud and I'm a Freudian or I'm not a Freudian: it's essentially, at this point in history, not a useful question. I suggested to some student, "don't be a Freudian, be a Freud." Just this seeking, onwards and onwards, and onwards. I think that we can assimiliate everything that Freud had to teach us, reject all the nonsense of which there was plenty, and then go on to the next story. Have I addressed myself to what you were driving at?

Southworth:

Yes, but I still have a question. Are there alternate ways to self-awareness that might be comparable to psychoanalysis?

Maslow:

Well, a friend of mine and I, we started to write a handbook of self-awareness. We were going to put together all these thousands of things, self-therapy, thousands of techniques that are available and we thought, and thought, and thought. And we decided on the first page to say just one sentence -- "If you would like to know yourself better, there is no substitute for a personal depth analysis." "But if you cannot afford this, if this is not practical, then we can suggest all sorts of substitutes and secondary means. They are not as good. You do need a professional to help to get the fullest in self-knowledge because of the facts of repression. It is very hard to break through your own repressions." I have no doubt, I have never run across any human being who didn't have trouble. We insist on fooling ourselves, making ourselves look better than we are, ducking our evil, but we have all got it. And for this technique of free association, whether you call it Freudian or not, I have not yet run across any good substitute.

Lichterman:

In line with this, some of us were discussing this problem, and it is quite obvious that for the overwhelming majority of counselors this is not a possibility in terms of a full psychoanalysis. Wouldn't it be desirable, on the other hand, as part of the training, at some point early in the training, and as part of the selective process as well, to have some kind of therapy or analysis, perhaps on a group basis?

Maslow:

Well the "T" group kind of a thing, the sensitivity kind of a thing. This suggestion about personal psychoanalysis simply isn't practicable, it is just too darn expensive, there aren't enough people - and half of them are no good

anyway. The psychoanalysts are a very mixed group of people, and it is a dangerous instrument. You run across a Dr. Blauberman every once in a while. I can't really recommend it except, you know, it is like saying to the poor man with tuberculosis, "You should give up your job and go to Florida." Well, we must work out all sorts of substitutes and the "T" group thing, the group therapy, the leaderless group kind of thing can achieve something. The more sensitive person can achieve the benefits of psychoanalysis, of awareness -- at least you get that psychological feeling, even if you don't know, that something is cooking inside of you. If you don't know what it is, you should know that it is there. And to give up the ultimate arrogance of the man who is totally rational.

Meyer:

It seems to me that one of the basic problems of the whole psychoanalytic approach has been to secretize the fulfillment of the individual, and I think that one of the things that needs to be done is to bring this out into the open where it is not a secret. It is not a one-to-one relationship all the time, but it is a part of life. And the psychoanalyst perpetuates this kind of mystique -- good mystique and bad mystique.

Maslow:

Well, I am at perpetual war with the psychoanalytic institutes, and in spite of what I have said, it would be as if I felt the doctrine, the methods, were in the hands of corrupt priests, you might say -- something like that. They have something precious and they use it badly. When I came to Brandeis I tried to bring it under the academic roof. I tried to establish an institute for the training of psychoanalysts, and I hoped to take it away from the doctors, who are a very bad group to handle this stuff. Well, I was beaten down by a local AMA, and they got after our president and I couldn't manage it. To give this kind of training which they persist in calling "medical treatment" -- this is our job, it is still our job. You can call it by other names, call it "counseling" if you like, or call it "guidance" if you like, call it whatever you do like, or call it whatever the state laws permit (laughter), and yet the fact remains that our job as I see it very, very clearly, our job is to approach another human being as if he were already human and not as if he were clay that we were going to mold and change, and it is our job to help him to discover his own self, his own potentialities, what's inside of him. This means that we have to be decent people; it means that we can't gobble up other human beings; it means that we must be horrified by the thought of breaking their bones, of breaking their spirits. We must help them to grow their own spirits which they have, all of them, in some kind of crippled form. Once you say that, you get into the whole thing -- that we must assess ourselves as to whether we are good priests, you know? And then you have to do all sorts of cleansing ceremonies for yourself, develop certain kinds of humility, certain kinds of arrogance also, self-confidence; you must take on your shoulders great tasks! Well, how to take on huge godlike tasks and yet remain humble? It is quite a job. But we must do

it, we have to do it! Who else? Who else do you give it over to? Doctors? Physicians? This is our fate.

Southworth:

There is something in our work that I thought was very important. In our work we have very open, very free-wheeling clinical case conferences. They are very intense. They are only once a week, but they seem to me like very wonderful learning experiences. The kind of thing that we do day by day with students.

Maslow:

Well it is exceptionally important for the intellectuals, you know, the more abstract; I sail off into the abstract realms in speculation, and I find that to keep my feet on the ground it is best for me always to keep in touch with the raw material. I read case histories -- for instance, like Gordon Allport's thing that has "Letters from Jimmy." This keeps you from getting too flossy -- or in any case material. I traipse around over the whole Boston area for every psychiatric residency program wherever a case is being discussed and I will sit in on it. But I think especially for the abstract and intellectual this is most important. This is a danger I think for all of us, especially if you start writing, theorizing -- it is to lose touch with the raw materials, with the humaness. Sure, I wish we could do this. You see, this is the one advantage the psychoanalytic institutions have. Their teaching is lousy generally, doctrinaire...but the one thing that they have which we don't have, which we don't run in the academic institutions is the continuous case seminar in which you follow along in great detail with the same one person who is responsible for reporting every week for six months.

Participant:

Rudolph Ekstein has done a great deal in this area. In your experience has it been easier for self-actualized persons to become self-actualized if they are single or if they are family men? (Laughter)

Maslow:

Well, the people that I have worked with are all older people.

Same Participant:

We were discussing yesterday the tremendous commitment in time that the self-actualized person must make to these areas. And what did this mean for his family situation?

Maslow:

I don't want to make any laws about it, it just happens. Somebody now is doing a study with Catholic religious people. They have no families in the usual sense, and what is the meaning of self-actualization in that context? I wouldn't make a rule about it. I think that they, in general, self-actualizing people, are professional people, and it is hard for me to think of a grown man who is without a woman to love. He would be missing too much. Well, I gather you can also love an institution, you can love a cause, we will find out; it is an empirical matter.